very approximate reproduction of this effect, whereas I have heard an expert break down hopelessly over a similar passage at an exhibition recital of a commercial machine.

With regard to the connection between tone quality and touch, I do not think Dr. F. J. Allen's explanation meets the case. Unless there is distinct experimental evidence to the contrary, I do not think we ought to exclude the *possibility* of a double or multiple impact between the hammer and string; indeed, some of my experiences favour this hypothesis. But I am inclining more and more to the belief that the differences may be largely due to the elasticity and inertia of the stem of the hammer. The introduction of these elements converts the hammer into a dynamical system capable of independent vibration. The method of normal coordinates then enables us to represent this system by a simpler system having the same vibration periods; for example, a system of two or more particles connected by elastic springs and moving in a straight line. It is clear that the duration of contact of a pair of such particles with the wire will depend largely on the state of compression between them and their relative velocity at the instant of impact. The interval between the release of the hammer and its striking the wire is probably short compared with the time of a free oscillation of the hammer itself, and certainly short compared with the time in which such an oscillation would die out. On setting the hammers of the horizontal piano low down, the variations of tone quality entirely disappear, as one would expect.

I find another favourable condition by developing Kaufmann's method in connection with the problem of a single inelastic particle striking a wire near one end. The duration of contact is determined by the vanishing of a function which has one or more minimum values before it actually vanishes, some of these being small. A very small difference in the assumed conditions might therefore convert one of these minimum values into a negative value. Remembering that such assumed conditions are probably not even approximately satisfied in practice, we still have a result indicating that the pianoforte hammer and string may be highly susceptible to any cause which tends to vary their duration of contact.

I am specially pleased to receive Dr. Heaviside's views on this subject, and to find that he has been long interested in these difficult problems.

Since writing this I have read Prof. Morton's letter, and am very glad to receive his references to previous work on the subject. With regard to his own experiments, I think something more is necessary for my purpose than what he mentions in NATURE, namely a comparison of the striking velocities of pianoforte hammers in *different* parts of the scale. I notice Prof. Morton does not mention what particular notes were struck in his observations. It would also be important to compare the striking velocities for two notes, one in the treble and one in the bass, when simultaneously excited by a common pneumatic impulse of long or short duration, such as can be produced in these piano-player experiments with properly cut chords. With regard to the other question, I think it is unfortunate that authors like Matthay have used the terms "good" and "bad" touch in this connection. I freely admit that the heavy, inelastic impacts produced by the inexperienced performer produce such a harsh effect as to be very objectionable (and possibly this may be due to Dr. Allen's so-called "xylophone" effect); on the other hand, any playing sounds to me mechanical which does not involve considerable variation of tone quality. This appears to me to be particularly necessary in studying piano arrangements of orchestral music, where the sharp, metallic tones of the brass instruments have to be brought out in contrast to the softer tones of the strings. Possibly when fingers are used the pianist usually has too many other matters requiring his attention. But whether a metallic effect is "good" or "bad" must depend on how and when it is used, and personally I should think a constant tone quality the worst effect of all. Prof. Morton's letter, however, raises a number of other questions which would take a long time to answer, and may have to be explained in subsequent correspondence.

G. H. BRYAN.

Plâs Gwyn, Bangor, North Wales.

Mackerel and Calanus.

WE all believe that most of our common food-fishes at some stage of life feed upon plankton, but those who have looked into sea-fisheries questions know that there is a great want of actual observations connecting the occurrence of some planktonic organism in quantity with the presence of a particular fish. Consequently the following record may be of interest to both marine biologists and fisheries experts.

We are out on a scientific fisheries cruise, and in addition to members of my own family, two well-known naturalists, Prof. Newstead and Mr. Alfred O. Walker are with us on the yacht, and we have just had what we regard as a satisfactory demonstration of the connection between a large shoal of mackerel and the occurrence of *Calanus finmarchicus* in unusual quantity.

On arriving in this bay last night we found that the local boats had been catching abundance of mackerel close to. We bought some for supper (good fish for a halfpenny each), and on dissection found that the stomachs of all of them were crammed full of fresh-looking Calanus (the individual Copepods being for the most part distinct and perfect), along with a few immature Nyctiphanes and larval Decapods. Prof. Newstead and my daughter then noticed, while fishing over the side of the yacht, about 8 p.m., that the gulls in the bay were feeding in groups around patches of agitated water evidently caused by shoals of fish. On rowing out to these we saw distinctly the mackerel, large and small, darting about in great numbers in the clear water, and we also noticed every here and there on the smooth surface of the water—it was a beautifully calm evening—innumerable small whirls or circular marks which, on looking closely, I found to be caused by large Copepoda close to the surface.

About twenty years ago I sent a note to NATURE, from the yacht *Argo*, in regard to large Copepoda (I think it was Anomalocera on that occasion, and the locality was further north, off Skye) splashing on the surface so as to give the appearance of fine rain; and this present occurrence at once reminded me of the former occasion, but here the Copepod was *Calanus finmarchicus* of large size and in extraordinary abundance. They could be clearly seen with the eye on leaning over the side of the boat, a small glass collecting jar dipped at random into the water brought out twenty to thirty specimens at each dip, and a coarse grit-gauge tow-net of about 34 cm. in diameter caught about 20 cubic centimetres of the Copepoda in five minutes. The mackerel were obviously darting about, occasionally leaping to the surface (which gave the gulls their opportunity) where the whirls caused by the Copepoda were thickest, and an examination of the stomach-contents of the fish on the yacht afterwards showed us that the amount in one mackerel was about the same quantity as that caught by the tow-net in five minutes. Prof. Newstead and I have made a count of 8 c.c. of the tow-net gathering, and estimate that it contains about 2400 specimens of

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Calanus. This would give about 6000 Copepods in the stomach of an average mackerel, or in a five minutes' haul of the tow-net, on this occasion.

It may be added that these mackerel were evidently not being nourished in accordance with the views of Pütter, and were clearly able to fill their stomachs from the plankton around them.

W. A. Herdman.

S.y. *Runa*, Tobermory, N.B., July 12.

Helium and Neon.

The experiments communicated to the Chemical Society recently by Prof. Collie and Mr. Patterson, the lectures delivered by Sir J. J. Thomson, and the discussions which have taken place in Nature, on the possible synthesis of the chemical elements have aroused great interest outside England. So far as I can ascertain, opinion is much divided. For my own part I may perhaps be permitted to say that I have always entertained the idea of a possible formation of elements of the helium group from other gases by *integration,* just as these are formed from other elements by disintegration (see *Chemical News,* 1896, and *Berichte,* 1899). When I put forward this view objection was taken that $4H$ is greater than He, 4·032 instead of 3·99, and the same kind of objection may be raised to-day that $He+O$, or $3{\cdot}9+16$, is less than Ne, 20·2 (unless Ne is a mixture of gases).

In order that the above question might be solved definitely, I would beg to suggest that experiments should be conducted in Röntgen-tubes from the electrodes of which every trace of the gases "occluded" or firmly held by them would be first removed by continued bombardment with kathode rays.

As regards the question put forward by Sir J. J. Thomson, whether the new gas X_3, discovered by him, may be a new element that fills the vacant space in VII. group, 1 series (VII–1), in Mendeléeff's periodic system, I may be allowed to remark that Mendeléeff's prediction of the properties of the elements Sc, Ga, Ge, could be successful, because it was an *interpolation*; whereas the prediction of the properties of the element $X=3$ includes an *extrapolation,* which is always rather uncertain; besides, the gases of the helium group were unknown at the time of the prediction. Its properties may be derived from the following equations:—(1) $Ne:F=He:X$; (2) $Li:F=H:X$; (3) $Li:H=F:X$; but also (4) $Fe:He=Mn:X$, and (5) $Cu:H=Br:X$, showing how uncertain the prediction of its properties becomes, so that it is indeed probable that it will be more negative than fluorine, but not necessary that the gas should combine with the silicon of the glass.

The delicacy of Sir J. J. Thomson's new method has superseded our old methods of investigation in a way similar to that based upon radio-activity, and the results of the study of the new gases discovered by this new method are awaited by chemists with the greatest interest.

Bohuslav Brauner.

Bohemian University, Prague, July 6.

Red Water and Brine Shrimps.

By the kindness of Mr. A. W. Sheppard and Prof. A. Dendy, F.R.S., I have been enabled to examine specimens of the brine shrimps from Geelong mentioned by Mr. Whitteron in his letter (Nature, June 12, p. 372). They belong to the species *Parartemia zietziana*, described by the late Mr. O. A. Sayce in 1903 (Proc. Roy. Soc. Victoria, xv., part ii., p. 232). In Parartemia the unpaired uterine sac is produced into two large dorso-lateral lobes lying on either side of the "tail," and appearing, as Mr. Whitteron says, "like the egg sacs of Cyclops." Mr. Sayce's specimens were obtained from a "brackish-water swamp near Lake Alexandrina, South Australia." It is interesting to learn that the species is able also to live in the brine of salt-pans.

The flagellate described by Mr. Whitteron is probably allied to, and perhaps identical with, *Dunaliella salina,* which has long been known to cause a red coloration in the brine of salt-pans in Europe and Algeria. A detailed account of this form and references to the somewhat extensive earlier literature of the subject are given by Clara Hamburger ("Zur Kenntnis der *Dunaliella salina,*" *Arch. Protistenk.,* vi., 1905, p. 111).

W. T. Calman.

British Museum (Natural History),
Cromwell Road, London, S.W., July 12.

The Maximum Density of Water.

Physiographers lead us to believe that the earth is defended from a profound glaciation, cumulative from year to year, by the law that water is heaviest at a temperature of four degrees above centigrade zero. If the main cause lies here, it is desirable that this measure should have its peculiar power set forth with more precision than has been customary.

The matter usually presents itself to students rather differently. The predominant fact is the floating power of ice. Hereby the water is screened from further attacks of the cold air, and dispersal is provided in the puzzling conditions of ground or anchor ice. Next perhaps in importance is the slow conduction of cold by water. Then comes the large value of the latent heat of water. It is not obvious why there should be disastrous results if the maximum density of water were at 0° C. The four units may be viewed as a helpful margin of safety rather than as an essential; but they would appear to be negligible in comparison with the 79 units of latent heat. Water at 0° C. is by no means unstable; each gram weight as it passes into ice throws out amongst its neighbours an amount of heat which is an effective safeguard against sudden and extensive solidifying.

W. B. Croft.

The College, Winchester, July 5.

Radio-activity and the Age of the Earth.

I am gratified to learn from Dr. Fermor's letter in Nature for July 10 that there is a scientific possibility of conceiving how the interior of the earth may be devoid of radio-activity. But if "high pressure and temperature" can inhibit the dissociation of "potentially radio-active" substances, will they not do so also in the interior of the stars? If so, radio-activity will no longer be available to prolong their radiation of energy, and we shall be back in the old difficulty about the age of the sun. Indeed, it will be aggravated, because we now have positive evidence for a high antiquity of the earth, while still unable to explain that of the sun.

F. C. S. Schiller.

Corpus Christi College, Oxford, July 11.

THE GENERAL MAGNETIC FIELD OF THE SUN.[1]

Those who are familiar with Prof. Hale's brilliant discovery of magnetic fields in sunspots, and are aware of the difficulties connected with that investigation, will greatly admire his courage in seeking to establish the much weaker general magnetic field of the sun itself. The following condensed account of the method adopted and results obtained is given, to some

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[1] Based upon an advance proof of a paper by Prof. G. E. Hale which is to appear in *The Astrophysical Journal.*

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